

# THE STUDY OF THE CAUSES OF WAR, WITH SPECIAL REFERENCE TO HEAD-HUNTING RAIDS IN BORNEO<sup>1</sup>

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## ABSTRACT

A study of the ethnohistory of head hunting among the Ibans of Borneo suggests that the biological or adaptive significance of particular behavior, that is, its value for the survival and reproduction of the people engaging in the behavior, should be taken into account in analyses of the causes of war. Care should further be taken to decide whether it is with the beginnings or with some other stage of the evolutionary development of war that we wish to deal, a conclusion which may apply as well to the study of causes of cultural phenomena in general.

What conceptual distinctions are useful in the study of the causes of war? In his *Science of Culture*, Leslie White says the following:

Wars are fought between societies, between sociocultural systems, between tribes and nations. It is the culture of any situation that determines whether warfare shall be engaged in or not, and if so how, with whom and for what . . . Warfare is a struggle between social organisms, not individuals. Its explanation is therefore social or cultural, not psychological.<sup>2</sup>

White's theme is amplified in W. W. Newcomb's examination of the causes of Plains warfare. Newcomb concludes:

Plains tribes did not habitually engage in war because individual men were "warlike." Individual men were warlike because their socio-cultural systems obliged them to be. The individual attitude of war was an expression of the socio-cultural process, by no means its cause or initiation . . . [It] does not matter for what reason the individual thinks he is fighting, and dying, as long as he is satisfying the needs and imperatives of his culture.<sup>3</sup>

The theme finds expression again in H. Clyde Wilson's critique of Robert Murphy's discussion of Mundurucú warfare.<sup>4</sup> War, says Wilson, is "a cultural

mechanism by which the sociocultural system attempts to adjust to its environment and has to be understood in cultural terms rather than as a psychological need.”<sup>5</sup> In the Mundurucú case, the sociocultural system, according to Wilson, “carried on extensive warfare in precontact times to recruit new members into society.”<sup>6</sup>

Wilson’s comments prompted a reply from Murphy which raises fundamental questions about the utility of the psychological-cultural dichotomy espoused by White and his students. Says Murphy:

Let’s clear this up immediately. Systems can be said to have certain concomitants and needs for their continuity and operation, but no sociocultural system has ever been known to think or locomote, let alone carry on extensive warfare. Only people do that. I am afraid that “the general confusion between cultural and psychological interpretations” exists primarily in the minds of those who believe that such a simplistic dichotomy is methodologically adequate.<sup>7</sup>

On the methodological inadequacy of the cultural-psychological dichotomy, I would tend to agree with Murphy. It seems to me to be hardly more illuminating to observe that Plains Indians or Mundurucú Indians went to war because their cultures were warlike (or because their sociocultural systems carried on warfare) than to observe that the Indians went to war because they themselves were warlike. What are the cultural “imperatives” that Newcomb speaks of? How do they arise? Why should the “sociocultural systems” referred to by Wilson attempt to adjust to their environments? These are some of the questions left unanswered by the use of the cultural-psychological dichotomy.

What alternatives to the use of this dichotomy do we have? In rejecting the dichotomy, need we accept the premise implicit in George Fathauer’s discussion of Mohave warfare, namely, that the one proper way to study war is to determine the values that are motivating members of a society to engage in it?<sup>8</sup> Or might it serve us better to adopt Marian Smith’s view that the motivations of warriors should not be distinguished at all from other “causes” of war, that making such distinctions is misleading and artificial when data show the motivations and the other causes to be operating simultaneously?<sup>9</sup>

Fathauer’s and Smith’s views are not the only alternatives. It seems both possible and useful to make a distinction between, on the one hand, the factors motivating the behavior of individuals and, on the other hand, the biological or adaptive significance of particular behavior, that is, its value for the survival and reproduction of the people engaging in the behavior. Such a distinction would be related less closely to the cultural-psychological dichotomy than to the distinction made by some biologists between ultimate and proximate factors or between adaptiveness, directiveness, survival value, ecological functions, and so on, *and* causation in the individual organism.<sup>10</sup>

I shall try first to illustrate the distinction by reference to data on warfare among some Borneo head-hunters and then to assess its usefulness in comparison with the usefulness of the cultural-psychological dichotomy.

## I

The “most inveterate head-hunters” of Borneo have been identified as the Ibans or Sea Dyaks of Sarawak.<sup>11</sup> They have been described also as “the wickedest head-hunters . . . perhaps in the whole world.”<sup>12</sup> It was, according to some sources, the desire for heads that made Iban warriors take part in fighting. Says Charles Hose (emphasis mine):

An attack upon a house or village by Ibans is usually made in very large force; but the party is more of the nature of a rabble than of an army, each man acting independently. *They seek above all things to take heads, to which they attach an extravagant value; and they have been known not infrequently to attack a house and kill a large number of its inmates for no other motive than the desire to obtain them.*<sup>13</sup>

Why the Iban passion for heads? To Hugh Low in the 1840's, the Ibans themselves accounted for it by saying merely that it was “the adat ninik, or custom of their ancestors.”<sup>14</sup> Raymond Kennedy, a more recent student of Borneo cultures, finds the reason for head-hunting to lie basically in the people's ideas concerning the magical power of human heads:

A Borneo settlement, let us say, has been suffering from epidemics, crop failures, and infertility of women. Casting about for a reason to explain their ill-fortune, they arrive at the characteristically Indonesian notion that their group lacks magical power. Their spiritual “juice” is running low. What they need is a fresh influx of supernatural vigor, not only to strengthen themselves, their crops, and their women, but also to fight off evil spirits with greater effectiveness.

One of the most direct means of getting the magical power they need is to capture a new batch of heads from some other group. The spiritual energy of the other settlement is most richly concentrated in their heads, and by getting some of these the home village will divert a part of the current vitality into their own community.<sup>15</sup>

Additional incentive for head-taking by the Ibans and for the warfare that it implied is said to have been provided by the women. According to Charles Hose and William McDougall, Iban women urged the men on and made much of those who brought heads home;<sup>16</sup> sometimes a girl would taunt her suitor by saying that he was not brave enough to take a head.<sup>17</sup> Odoardo Beccari, whose wanderings in Borneo took place in the 1860's, states also that an Iban would often start on a head-hunting expedition by himself, “as a relaxation or to wear off the effects of a domestic squabble, just as with us a man might go out rabbit-shooting to get over an attack of ill-humour.”<sup>18</sup>

Such things as the desire to follow ancestral custom, to obtain magical power, to release tensions, or to have heads in order to get women may be regarded as values motivating Iban men to hunt heads and to fight. However, to point to these and similar factors motivating the behavior of individuals is to provide only a partial answer to the question of the causation of head-hunting and warfare

among the Ibans. For a fuller answer, an examination of how—if at all—Iban warfare may have contributed to the survival and perhaps the increase of the Iban people is, I will argue, also required. I say “how—if at all” advisedly, for I should not want to be forced into defending the extreme and plainly absurd position that warfare among any people *must* contribute to their survival or increase; warfare obviously does not do this for a people who become extinct by virtue of aggressions directed against them. However, in the Iban case, it is possible to indicate ways in which adaptive advantage may have accrued to the people through warfare.<sup>19</sup>

It seems clear that by means of head-hunting and warfare the Ibans acquired land. According to J. D. Freeman, the Ibans during the last century were “in a very real sense . . . fighting for the possession of new territory.”<sup>20</sup> They not only took heads but also burnt down the long-houses of the enemies, threw their iron implements into nearby rivers and took away their wives and children.<sup>21</sup> In these ways the Ibans discouraged the enemies and made them withdraw from their land and leave it for exploitation by the Ibans.<sup>22</sup>

Did the acquisition of new land contribute to the survival and increase of the Iban people? There are indications that this was the case. Charles Brooke, who eventually became the second rajah of Sarawak, made an expedition into the interior of Sarawak in 1861 and noted the following in his journal:

This Katibus river must muster about 10,000 souls; and here is one instance of the rapid increase of inhabitants, as they came here from Batang Lupar, in which river there are more now than can use the land according to the Dyak [Iban] method of farming. And these inhabitants seem to be fast clearing every hill in the vicinity of the Katibus river.<sup>23</sup>

This Katibus region appears to have been only sparsely populated before the Ibans moved in, and the Kayans, another Borneo people, had some claim to the territory. The fact that there was fighting between them and some of the Iban pioneers raises some questions.<sup>24</sup>

Inasmuch as an estimate based upon a 1947 census gives a mean population density of less than twelve people per square mile for all of Sarawak,<sup>25</sup> it may be asked why the Ibans in the last century, when their numbers were smaller than now, should have taken land claimed by other tribes. If, as Brooke says, Iban population was increasing,<sup>26</sup> then why did it not spread peacefully into virgin territory not claimed by anybody? Part of the answer is that much of the virgin territory was poor land. Alastair Morrison, a recent commentator on agricultural problems in Sarawak, has observed:

The ratio of population to area in Sarawak is misleading. To the outsider . . . it appears that there are great reserves of land. In fact there is very little good land in Sarawak and that which is even passable is strictly limited in area. There exist instead great areas of swamp which are either completely useless for agriculture or can only be utilised after costly drainage schemes have been introduced. And behind the swamps there are thousands of square miles of broken, barren, steep hills, the ‘green

desert' . . . which in the light of present knowledge can never be made really productive.

At the same time there is land hunger in the relatively well populated Iban and Land Dayak areas.<sup>27</sup>

Moreover, it may be the case that for swidden (slash-and-burn) agriculturalists in the humid tropics in general it is advantageous to use second-growth land rather than primary forests, which are extremely difficult to clear. That this may have been true of the Ibans is something that I have tried to indicate in another place.<sup>28</sup> And if it is true, it may well have been a factor in the Ibans' taking previously farmed land from other groups at times rather than expanding into virgin territory.

Possible advantages of taking riverine land from other groups rather than using non-riverine virgin territory must also be mentioned. Throughout Sarawak, as in many other parts of the humid tropics, human occupation has, as E. R. Leach points out, taken the form of ribbon development along the banks of the rivers.<sup>29</sup> Hose and McDougall observe:

With the exception of the Punans and some of the Muruts who inhabit the few regions devoid of navigable streams, all the peoples of Borneo make great use of the rivers. The main rivers and their principal branches are their great highways, and even the smallest tributary streams are used for gaining access to their *padi* [rice] fields.<sup>30</sup>

The rivers were important also as sources of the fish that constituted a significant part of the indigenous diet.<sup>31</sup> Furthermore, as I have noted elsewhere, the rivers probably facilitated movement between communities of the same tribe so as to enable people to cooperate more effectively in military and perhaps also in subsistence undertakings.<sup>32</sup>

Another important consideration that I have discussed elsewhere is the readier access to trade goods that the riverine locations made possible.<sup>33</sup> By using the rivers for taking rice, rubber, and other products to the Chinese and Malay traders (or by being on the rivers so that the traders could come to them), the natives of the interior could get not only the treasured brassware and ceramics<sup>34</sup> but also guns<sup>35</sup> and the bars of European iron and steel from which were fashioned essential agricultural and fishing implements (chopping knives, fish spears and so on) and the swords and spears used in fighting.<sup>36</sup> It may be suggested that the readier the access to trade goods the more efficient would be the economic and military system;<sup>37</sup> the Kayans and Kenyahs, pushed farther into the interior by the Ibans, still needed at the end of the last century to work iron, no doubt laboriously, from ore found in the river beds and possibly also from masses of meteoric iron.<sup>38</sup> It may be said that by spreading aggressively along the rivers as Iban population was increasing, the Ibans were probably maintaining the conditions (including the relatively ready access to trade goods and the facility of intercommunity cooperation) under which further increase in numbers remained feasible for them. The process of warlike territorial expansion would thus have had considerable adaptive significance for the people.<sup>39</sup>

The head-hunting raids against the Kayans probably had one additional effect making life more secure for the Ibans. It must be noted that the nomadic Ukits were "inveterate opponents" of the Ibans and were extremely difficult opponents to deal directly with.<sup>40</sup> "We can never see or get near them," said the Ibans, "and we cannot resist an enemy like a bird that blows an arrow from a tree upon us."<sup>41</sup> These same Ukits, however, were on friendly terms with the Kayans and are reported to have taken every opportunity of supplying them with the heads of people, presumably mainly Ibans, obtained from parties in search of gutta-percha or other wild products. In exchange for these heads, the Ukits, who did not as a rule cultivate the soil, got rice and sago from the Kayans.<sup>42</sup> It may be suggested that the Ibans, although unable to deal with the Ukits by using force directly against them, could nevertheless reduce their raiding effectiveness by driving back the more sedentary Kayans with whom the Ukits had a symbiotic relationship.

## II

We see then that Iban warfare appears to have had effects significant for the survival and increase of the Iban people. It is likely that in this brief discussion I have pointed to only some such effects and there are others that remain to be noted. However, my interest at this point is not in each and every one of the effects but rather in their general character and in the very fact that I have been able to suggest such effects at all. Had I accepted Fathauer's premise that the one proper way to study warfare is to determine the values that are motivating members of a society to engage in it or had I, like Marian Smith, not bothered to distinguish the motivations of warriors from other kinds of factors, I might have been satisfied to note simply that the Ibans fought in order to obtain magical power or in order to have heads so as to be able to get women. One measure of the value of conceptual distinctions is whether they make us look for data that might otherwise be ignored, and the distinction between the factors motivating the behavior of individuals and the biological or adaptive significance of behavior does, as we have seen, make us look for new data.

It may be asked then whether the cultural-psychological dichotomy would have had the same result. I think not—or, at least, not necessarily. In terms of the dichotomy, some of the things that we have treated as factors motivating individual warriors would have to be regarded as cultural rather than psychological factors, for culture, according to Leslie White, consists of events, including ideas, beliefs, languages, tools, utensils, customs, sentiments, and institutions, dependent upon man's ability to use symbols.<sup>43</sup> By this definition, ancestral custom and the ideas concerning the magical power of human heads are part of the culture of a situation, part of a socio-cultural system to which might be attributed the power—see the earlier citations from White and Newcomb—to oblige individuals to do things. A cultural (or "culturological") interpretation of Iban warfare would thus appear to be possible without any reference to the role

of European trade goods or to the displacements of tribes and the acquisition of new agricultural land by the Ibans.

Granted, however, that the distinction between individual motivations and adaptive effects does lead us to data that might otherwise be ignored, it may still be asked why these data should be important. What relevance do *effects* of warfare, even if they are adaptive, have to the study of the *causes* of war?

I would say that the effects, on account of their adaptiveness, can become causes of the continuation and spread of warfare. One way to describe the continuation and spread of any cultural activity is by reference to the numbers of those doing it relative to the numbers of those not doing it, and there are two means by which a relative increase in the number of the "doers" can come about. One of these is through differences in the rates of survival and reproduction of doers and non-doers. In the case of the Borneo tribes, it would seem that the Ibans, "doing" a certain kind of warfare and having more good land, better tools to exploit it with, better weapons perhaps, and perhaps a greater number of captured women to bear children for them, had a higher rate of survival and reproduction than their enemies. Maintenance and increase of the numbers of those practising the Iban brand of warfare could thus come about simply through the maintenance and increase of the numbers of the Ibans, who would in the course of their enculturation learn such customs of their ancestors as head-hunting and its concomitants. The circular chain of cause and effect is readily seen: warfare has effects conducive to the survival and increase of the Ibans; the survival and increase of the Ibans are conducive to the maintenance and spread of Iban warfare.

The second of the means by which a relative increase in the numbers of the doers of some cultural activity can come about is through adoption or borrowing of the practice—through "non-doers" becoming "doers." It may be that the pressures for the adoption of enemy war practices tend to be especially great, since the non-doers can, at least in some situations, perceive destruction and death as the likely consequences of not borrowing the enemy ways. As Walter Goldschmidt remarks, it is a distinctive and important feature of cultural evolution that people will often take over from their neighbors those cultural elements which, by giving the neighbors some kind of competitive advantage, constitute a manifest threat to the people's own survival and well-being.<sup>44</sup> Frank Secoy's comprehensive study of rapidly changing military patterns in the North American Great Plains strikingly shows the speed with which people may adopt the superior fighting technique of their enemies; Plains tribes failed to make such adoptions only when they were prevented by "temporarily insurmountable, external political or trade barriers"—and the tribes in such cases were either destroyed or forced into a territorial retreat.<sup>45</sup> For example, when their being cut off from all sources of guns prevented the Apache Indians from adopting enemy war practices involving the use of guns and horses, they were pushed from their territories and their numbers apparently declined.<sup>46</sup> The fact that the Kayans of Borneo could be characterized as "perhaps, the least aggressive of all

the interior peoples with the exception of the Punans"<sup>47</sup> and that they were unable in the long run to resist the Iban encroachments on their territory may be due in part to their not having been able to adopt those Iban war practices which depended, in one way or another, upon the readier access to trade goods that the Ibans had.

It needs to be emphasized that in the situations where there is "borrowing" of war practices certain adaptive effects of warfare may still be part of a circular causal chain. I would not say that this must always be so, and, indeed, I shall refer later on to the Ibans' borrowing of certain war practices which, at the time of adoption by the Ibans, may have had no very significant adaptive effects at all (although ultimately they apparently did have such effects). Just here, however, I am interested in cases of borrowing where adaptive effects *can* be indicated.

The warfare practiced by our enemies, let us say, is conducive to *their* survival and increase at the expense of our own. It is this that makes the enemies a threat to us and to our institutions. We recognize the threat and respond to it by adopting the warfare of our enemies. As a result we survive. But, until and unless some equilibrium of peaceful co-existence with potential enemies is attained, we survive now as a "warring" people, whereas before the enemy threat arose we may have been a people of peace. A cause of the warfare by us may then be said to be the fact that the warfare by our enemies had adaptive effects for them. The effects are relevant to the study of the causes of war.

### III

If we now rest the case for looking at or looking for the adaptive effects of warfare in the course of the study of the causes of war, we can ask again about the factors motivating the behavior of individuals. Have we distinguished them from the adaptive effects merely in order to eliminate them from consideration as "real" causes? Do we want to say, as Newcomb has said, that "it does not matter for what reason the individual thinks he is fighting, and dying . . ."?<sup>48</sup>

I think it does matter, at least in some contexts, and my reasons for thinking so underscore some important differences between White's theory of cultural evolution and other theories.

In the course of the last few remarks, I have quoted Newcomb out of context. The complete sentence from which my citation was drawn reads as follows: "Yet it does not matter for what reason the individual thinks he is fighting, and dying, as long as he is satisfying the needs and imperatives of his culture."<sup>49</sup> This notion of the imperatives of a culture seems to me to be related to—possibly to derive from—White's conception of cultural evolution as not only a determinative but also a highly predictable process. In White's view, when there is war, it is because the development of technological, economic, political, and military forces have made it inevitable.<sup>50</sup> The thoughts and feelings of individuals do not matter; what culture needs, culture gets.

In contradistinction to this view is one that sees cultural evolution operating



in a less predictable or predetermined manner. In this other view, cultural inventions or innovations would appear to be taking place more at random than White suggests, and the spread or maintenance or elimination of the inventions would appear explicable mainly in terms of their advantages under the conditions in which they occur. There is, as I have remarked elsewhere, no insistence in this view on the possibility of predicting either the occurrence of particular cultural innovations or the interaction of culture with particular environments, although it is recognized that similar innovations *may* arise in similar environments and then be similarly favored in the selective process.<sup>51</sup>

What do these contrasting views imply for the interpretation of warfare among the Borneo tribes? Newcomb concluded his study of Plains warfare by saying that the "Plains cultures were warlike because they had to be,"<sup>52</sup> and we may suppose that Iban culture could likewise, in a manner consistent with White's view of cultural evolution, be interpreted as warlike because it had to be. No such imputation of necessity or inevitability is entailed by the other view of cultural evolution. The other view allows us instead to note, where data are available, the particular conditions under which the Iban pattern of fighting arose and to note that we may call, for the want of a better term, the role of "historical accidents" in its origination. Thus we can cite evidence on the recency of the custom of head-hunting among the Ibans; the indications that the Ibans took it over from other tribes among whom it had been established since an earlier time, although in a different ritual context; and the indications that the Ibans were encouraged in head-hunting during the first part of the nineteenth century by the Malays who made a practice of rewarding their Iban associates in piracy (prior to its suppression by Rajah James Brooke) with the victims' heads, as well as sometimes with a share of the plunder and slaves.<sup>53</sup> What needs to be emphasized about these various circumstances for purposes of the present discussion is that in the early years of Iban head-hunting the thoughts and feelings of Iban individuals can be argued to have indeed mattered. If, as I think is the case, we cannot insist that the adaptive effects conducive to a continuation and spread of Iban warfare were operative from the outset, then whatever it was—whether Malay influence or something else—that made the Ibans in the beginning *think* that head-hunting was a worthwhile thing to do and the very fact that they did think so may be important. It can be argued that had they thought otherwise and had they not taken up the practice—and it is possible that such would have been the case under only very slightly different historical circumstances—then the later development of Iban warfare, which we have attributed in part to its adaptive effects, might not have taken place.

These remarks are not intended as an abjuring of determinism. No doubt the thoughts, feelings, and actions of particular Ibans at particular times all had their causes. The notion needing to be abjured is that these causes were necessarily oriented towards the production of some particular eventual adaptive effects. Once we renounce *this* notion, we can recognize that there may be determination without predetermination and that there may be orderliness in cultural

evolution (produced by selection operating on the innovations that do in fact, under very particular historical circumstances, occur) and yet only a limited predictability. With special reference to Iban warfare, we can say that at those points in its evolution when it was still something new to the Ibans themselves the thoughts and feelings of the Ibans about its desirability may have been significant determinants of whether or not it would then become established among them.

At the same time, it must be remembered that the ultimate establishment of a particular kind of warfare among the Ibans is likely to be, as we have seen, explicable to a large extent in terms of the adaptive effects of that warfare. This suggests that before deciding to concentrate our attention on either the factors motivating the behavior of individuals or on the adaptive effects of behavior—or even on both at the same time—we have to decide whether it is with the beginning or with some other stage of a particular evolutionary development that we wish to deal. This is a conclusion that may well be applicable to the study of the causes of cultural phenomena in general as well as specifically to the study of the causes of war.

#### NOTES

1. An earlier version of this paper was presented at the 58th annual meeting of the American Anthropological Association, in Mexico City, December, 1959. I thank the Social Science Research Council for a grant-in-aid that enabled me to do research in England on Borneo warfare.
2. White (1949: 131-132).
3. Newcomb (1950: 329; also see 1960: 321 and following pages).
4. Murphy (1957).
5. Wilson (1958: 1194).
6. *Ibid.* p. 1195.
7. Murphy (1958: 1199).
8. Fathauer (1954).
9. Smith (1951: 359, note).
10. See Tingbergen (1951: 151-152).
11. Hose and McDougall (1912: I: 187).
12. MacDonald (1956: 10).
13. Hose (1926: 144). And compare with Marryat (1848: 81): "The great object in their [the Ibans'] combats is to obtain as many heads of the party opposed as possible. . . ."
14. Hugh Low (1848: 188-189).
15. Kennedy (1942: 101).
16. Hose and McDougall (1912: I: 76, 186-187).
17. See Beccari (1904: 47); Hornaday (1890: 465); Keppel (1847: I: 55-56; 1853: I: 129); Moor (1837: 9 [note], 52); Roth (1896: II: 163 and following pages).
18. Beccari (1904: 47).
19. By "adaptive advantage," I mean advantage for the people's survival and reproduction and I mean nothing else. The various embroideries on this fundamental meaning have, I believe, resulted only in needless confusion (see Harris 1960).
20. Freeman (1955: 25, note).
21. *Idem*; Keppel (1847: I: 256).
22. Freeman (1955: 11 and following pages, 25-26 [note], 111); see Vayda (1961: 353).

23. C. Brooke (1866: II: 192).
24. *Ibid.*, p. 248; Freeman (1955: 13 and following pages).
25. Noakes (1950: 17).
26. C. Brooke (1866: II: 192, 335-336).
27. Morrison (1957: 33).
28. Vayda (1961: 354).
29. Leach (1950: 9).
30. Hose and McDougall (1912: I: 131).
31. *Ibid.*, p. 134; Hose (1926: 98).
32. Vayda (1961: 355).
33. *Idem.*
34. Freeman (1955: 76-77).
35. Cunyngname (1892: 77); Grant (1885: 17); Maxwell (1881: 87).
36. Horsburgh (1858: 37); Hose and McDougall (1912: I: 193 and following pages); Hugh Low (1848: 158, 209-211).
37. See Skelton (1871: 1): "I shall not make an exaggerated assertion when I say that among the Dyaks the best head takers are in nearly all cases the best traders. . . ."
38. Hose and McDougall (1912: I: 193-194).
39. Throughout the discussion I am, like Freeman (1955: 13), accepting the evidence recorded by Brooke in the 1860's concerning Iban population increase. The possibility that the rate increase diminished in subsequent periods should be noted. St. John, also writing in the 1860's, gave the figure of 120,000 as a "most moderate" estimate of Iban population (St. John 1863: II: 308). Comparison of St. John's estimate with the 1947 census figure of 190,326 (Noakes 1950: 81), representing the number of Ibans in a Sarawak somewhat more extensive than the territory having the name in St. John's time, does not indicate an increase in Iban population at all comparable to the explosive growth that has taken place within the last hundred years among many other peoples of Indonesia and Southeast Asia. This suggests to Noakes (1950: 8), the Superintendent of the 1947 census, that "the transformation of the Sea Dyaks from a warrior race habitually engaged in bloody affrays into peaceful agriculturalists must have been accompanied by a decline in the birth rate or an increase in the incidence of disease." The size of the Iban population may of course have been exaggerated by St. John. According to one official report (Ussher: 1879: 379), the Sarawak Government in 1877 estimated Iban population at only 90,000.
40. Freeman (1955: 15); H. B. Low (1884: 31).
41. Cited in C. Brooke (1866: II: 250); see Roth (1896: I: 19, note).
42. C. Brooke (1866: II: 250).
43. White (1949: 15).
44. Goldschmidt (1959: 131); see Bouthoul (1951: 146 and following pages).
45. Secoy (1953: 94, 95).
46. *Ibid.*, pp. 81-84.
47. Hose (1926: 130).
48. Newcomb (1950: 329).
49. *Idem.*
50. White (1949: 14, 229-230).
51. Vayda (1959); see Goldschmidt (1959: chapter 4), and Murdock (1959).
52. Newcomb (1950: 239).
53. Beccari (1904: 47); Hose and McDougall (1912: I: 187-188); Keppel (1853: I: 128-129); Hugh Low (1848: 188-191); J. Brooke (1848: I: 384-385); St. John (1863: I: 4, 78-79); Singapore Commission (1854: 107).

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